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NOTES AND LITERATURE

RUSSO ON SEX-DETERMINATION AND ARTIFICIAL MODIFICATION OF THE MENDELIAN RATIOS

In the April number of the Naturalist Professor Jordan presents an interesting review of recent literature on sex-determination in the course of which he gives an extended account of Russo's[1] experiments with lecithin-fed rabbits. Great importance is attached to this work because it points to conclusions diametrically opposed to those reached within the last ten years by nearly every one else who has studied sex-determination either from the standpoint of the cytologist or from that of the experimental breeder. The nearly unanimous verdict has been that sex-determination is a matter of gametic differentiation, and that the sex of a developing organism is not influenced by conditions of nutrition either applied to it directly or brought to bear upon the mother. The only exception, apparent but not real, as I pointed out in 1903,[2] is afforded by organisms in which both parthenogenesis and sexual reproduction occur. Abundant nutrition favors parthenogenesis, scanty nutrition causes a return to sexual reproduction, including the production of males. Russo has revived the older idea that in non-parthenogenetic organisms also scanty nutrition is a cause of male-production. If true, this is a matter of the greatest importance, both theoretical and practical. Jordan is quite right in so regarding it, but has apparently failed to appreciate the uncritical character of Russo's evidence. This has, however, already been pointed out by several writers, most recently by Punnett,[3] whose brief but convincing paper seems not to have been seen by Jordan. Russo himself has never published the results of his experiments as a whole, but only of *selected experiments* the results of which were favorable to his thesis. This fact alone would throw Russo's claim out of an impartial court, but to be more than fair to Russo, it may be said that his experiments have been repeated twice independently, by Basile[4] and by Punnett, both times with negative results.

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[1] Russo, A., "Studien über die Bestimmung des weiblichen Geschlechtes," G. Fischer, Jena, 1909.

[2] Castle, W. E., *Bull. Mus. Comp. Zool.*

[3] Punnett, R. C., *Proc. Cambridge Phil. Soc.*, Vol. 15, Pt. 2, p. 92, 1909.

Russo's original evidence consisted of 100 *selected* litters of young borne by lecithin-treated does which included 217 male and 431 female rabbits, and of 100 control litters (likewise *selected*) from non-treated does which included 400 male and 287 female rabbits. The claim is made that lecithin treatment raised the percentage of female offspring from 41.8 to 56.5. Russo in this case proves too much, for his selected controls fall as much below the normal percentage of females as his selected lecithin-treated cases exceed it and, it is fair to assume, for the same reason, because they are *selected.* It is astonishing that a scientist should present such evidence. A prosecuting attorney who should be allowed to decide what evidence should be presented to the court, both for and against, and that all else should be excluded might convict any one of us of all the crimes enumerated in the criminal code.

On the other hand, Basile and Punnett give *all* their results, not selected ones; they mean to tell the *whole* truth, not selected truth. Basile obtained from lecithin-treated does 66 male and 51 female young, or 43.6 per cent. females, while from controls he obtained 225 male and 215 female young, or 48.8 per cent. females. This result is contrary in nature to that of Russo. If any effect is to be ascribed to the lecithin, it is in this case male-production rather than female-production as claimed by Russo. Punnett in repeating Russo's experiments followed methods outlined for him in detail by Russo. They are therefore particularly satisfactory. From lecithin-fed does he obtained 24 male and 23 female young, or 48.9 per cent. females; from controls 54 male and 49 female young, or 47.6 per cent. females. These proportions agree closely with those obtained by Basile, and accord with Hurst's extensive observations cited by Bateson[5] to the effect that "under normal conditions male and female birth are sensibly equal," in the case of rabbits. Finally Heape[6] has criticized on cytological grounds the supposed histological distinction between male and female ova of the rabbit as described by Russo. Heape shows that in all probability the fat-containing, supposedly female ova are degenerating ova.

[4] Basile, C., *Atti Acad. Lincei,* Vol. 17, p. 643, 1908.

[5] Bateson, W., "Mendel's Principles of Heredity," 1909.

[6] Heape, W., *Proc. Cambridge Phil. Soc.,* Vol. 14, p. 609.

If further evidence were wanting of the wholly uncritical character of Russo's work, it might be found in his analysis of the results of crosses involving Mendelian color characters. Jordan correctly states Russo's claim, though not his evidence. The claim is that when albino or spotted females were mated with gray or black males, the color character of the latter dominated in the young, but when the same females were first treated with lecithin and then mated with a black male the color character of the mother occurred in part or all of the young. The conclusion is drawn that lecithin treatment of the mother is a practical means of fixing a desired maternal character as a racial character, a claim so astonishing and so important, if true, that with Jordan's indorsement it is in danger of being taken seriously by readers of the NATURALIST. For that reason I have been prompted to examine Russo's evidence carefully and to write this note. His evidence consists of four cases, *all* the cases bearing on this point concerning which he gives detailed information, and he states that these are his *best* cases.

Case 1.—A young albino (Polish) female was treated with lecithin and mated with a gray male and produced two gray young. The lecithin treatment was continued and she was now mated with a black male and had eight young, six white and two black. The reader is led to conclude that the *further* lecithin treatment is responsible for the appearance of white young in the second litter. But is it? No evidence is given as to the gametic character of the male parent of either litter. If the gray male was homozygous he should have produced, as observed, only gray young, though the production of two young only was no adequate test of his character. The *same* male should have been used after the "further" lecithin treatment if information were desired about the effect of lecithin, but this was not done. A new untried male of a different color, black, was used. He produced both albino and black young, a result which indicates that he was heterozygous. If so, the result obtained is such as would have followed without any lecithin treatment whatever! The case proves absolutely nothing, because neither colored parent was tested, so far as we have any information. Russo seems to think that the first litter affords a criterion of what the second should give by a different male parent, a conclusion which no one familiar with Mendelian principles would entertain for a moment.

Case 2.—A young Himalayan (albino) female rabbit was treated with lecithin and mated with a black male. She produced by him three litters of young. The first litter contained six young, all black. The second litter contained four young, two of which were Himalayan, two yellow-and-white.[7] The third litter contained seven young, concerning the color of one of which no account is given; four were Himalayan, one yellow-and-white and one black-and-white. The case (a selected one) is supposed to show that continued treatment with lecithin produces more young of the maternal type in later than in earlier litters. But a *selected case* can not fairly be used to show anything of the sort. Only a fairly uniform result of repeated experiments could establish it. Under the circumstances we can only ask what the expected Mendelian result would be in a case like this if no lecithin treatment were involved, and then inquire whether the observed result differs in any respect from it.

If no lecithin treatment were involved we should say that the black male used was heterozygous in the recessive characters albinism, spotting and yellow; and the expectation would be that he would produce young of five sorts in one of the following distributions, depending on the gametic character of his albino mate:

Black		Yellow		Black and white		Yellow and white		Albino
1	:	1	:	1	:	1	:	4
3	:	3	:	1	:	1	:	8
9	:	3	:	3	:	1	:	16

The observed distribution for the three litters is 6 : 0 : 1 : 3 : 6. This is so good a Mendelian result that it would not call for any special comment in ordinary breeding work. Further matings would be called for, but it would be surprising if these did not supply the missing class, yellow, and give numbers agreeing more closely with one or another of the suggested ratios.

No control matings of either parent in case 2 are recorded by Russo.

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[7] Regarding this class Russo says they were of the same race as the mother but with yellowish or white ground-color (''von derselben Rasse, aber mit gelblicher bzw. weisser (wie in Fig. 27), Grundfarbe''). But the photograph, Fig. 27, shows unmistakably that these animals were not albinos at all but Dutch-marked colored animals, which invariably have dark eyes. A spotted dark-eyed animal is not by any means of the ''same race'' as an albino, as is well known to students of genetics. Spotting behaves as a Mendelian unit-character wholly independent of albinism.

Case 3.—The statements of Russo concerning this case are very incomplete. A spotted "Olandese" female after lecithin treatment was mated with a silver-gray male and produced three spotted and one self-colored young, which are figured. Before treatment with lecithin she had been mated under normal conditions (*once* with the same silver-gray male, "mit demselben grau Männchen einmal," but we are not told that he was the father of the young, we are rather left to infer that she may have mated with other males also. Five young were afterward born—"es erzeugte dann später 5 Junge," all gray. Again the female was mated under similar uncertain conditions, "unter den gleichen Bedingungen," and produced four gray young. Since in a third litter, after lecithin treatment, she produced spotted young, it is concluded by Russo that the lecithin treatment was responsible for the changed result. But before accepting this conclusion we should like to be assured that one and the same male was used in both cases. The silver male may have been heterozygous in spotting, the father of the earlier litters not.

Case 4.—Two Himalayan (albino) females, sisters, were mated with the same black male. One of the two was lecithin treated, the other was not. Both produced black young, five each. This looks like a bad case to select in support of lecithin influence, but we are told further that in the following year, when lecithin injections were employed forty days previous to copulation, a litter of two Himalayan young was obtained. The sire of the litter is figured but we are not told whether he was the *same* as the one used the previous season. This omission is significant. The male figured was evidently heterozygous in albinism, but it is a pretty safe guess that he was not identical with the male used the previous season, which was in all probability homozygous in black. In this case as in the others described by Russo it is impossible to avoid the impression that the author is not making a full and frank statement of facts but is stating half-truths likely to mislead the unwary. In Jordan's case this is exactly what has happened. He says: "The evidence here appears unequivocal that external conditions (*e. g.*, nutrition) can determine the kind of sex *and entirely vitiate the Mendelian scheme of ordinary crosses*" [italics mine]. I have, however, presented in detail *all the evidence* which Russo gives in support of the assumed vitiation of Mendelian inheritance. I think the reader will agree with me that the evidence is more than equivocal and that the vitiation is of reasoning not of inheritance.

So far as I know, only one other investigator besides Russo has laid claim to having modified the ordinary course of Mendelian inheritance by external conditions. Such a claim, if I rightly understood him, was made by Professor W. L. Tower, in a paper read at the last annual meeting of the American Society of Naturalists, in the case of beetles of the genus Leptinotarsa. Detailed information regarding the cases in question will be awaited with much interest. It is to be hoped that this will prove more complete and satisfactory than Russo's.

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HARVARD UNIVERSITY,
June 11, 1910.

THE BUBONIC PLAGUE

BUBONIC plague is primarily an animal disease. Its original victim is said to have been a species of rodents found in the mountains of Mongolia. Several Russian scientists have sought to establish this hypothesis, according to which the *Arctomys bobac,* a Mongolian marmot, is the primitive animal host. In it the disease is permanently prevalent, and from it both man and the rat are infected periodically. According to the view of these writers the final eradication of the plague from our globe would be accomplished by the extermination of this rodent. That this view is over-optimistic may be inferred from the probable existence of other ancient centers from which plague epidemics have originated, and in which, consequently, permanently infected animals are at home, and also from the recent origin of such an established center of animal infection on our own continent.

In the introduction to an article on Plague Eradication Measures (Squirrel Campaign) in California, Rucker[1] comments on the epizootic which for four years has been spreading among the ground squirrels of Contra Costa County, and which more recently has been reported from other districts also. The animal infected is *Citellus beecheyi,* which is reported by ranchers to have died by the thousands in 1904–5–6. In appearance and habits, it resembles closely the Thibetan marmot referred to above. Not until there came cases of human plague due to plague-infected squirrels was the disease of the latter subject

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[1] *Journal Amer. Med. Assn.,* Vol. 53, p. 1995.